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Book reviews

Heinz D. Kurz and Neri Salvadori (with C. Gehrke, G. Freni and F. Gozzi), *Interpreting Classical Economics: Studies in Long-Period Analysis*. Routledge Studies in the History of Economics No. 89. London and New York: Routledge, 2007. xiii + 269 pp. $135.00. ISBN 0-415-42880-7.

The character, content and meaning of 'classical economics' has been a subject of considerable, sometimes 'vigorous', debate (to put the point mildly) over the past four decades. One thinks, in particular, of what might be dubbed 'the Ricardo wars', in which prominent figures jousted over the character, content and meaning of the political economy of that great and remarkable man. It was of course Karl Marx who invented the category 'classical' economics (or more precisely, classical political economy), as a way of capturing what he perceived as the distinctly scientific element in the development of political economy, from William Petty and Pierre le Pesant Sieur de Boisguilbert to David Ricardo and Jean Charles Leonard Simonde de Sismondi. Now, there apparently are some who think that the very notion of a classical economics serves no useful purpose, being an alien ex post imposition, especially unacceptable when pressed into the service of that shocking (alleged) intellectual offence, 'rational reconstruction'. It will be a very 'interesting' intellectual history that has no recourse whatsoever to any terms that are not used by the actors under study themselves. Actually, it will not be history at all – supposing any such intellectual history can even be written. (For a much more considered and nuanced view of rational reconstruction, see Waterman 2003.)

The authors of the essays in this volume certainly believe that 'classical economics' has a valuable meaning, and integrity as a device for historical interpretation and reconstruction. The main inspiration for that view is Sraffa's distillation of an essential and unified theoretical structure that makes sense of the tradition of political economy from the seventeenth century to the nineteenth century, in particular, with regard to the theory of functional income distribution in terms of the division of a social surplus, and the theory of prices associated with that approach to distribution. There is obviously some connection between this project of Sraffa and his extraordinary scholarly edition of Ricardo's works and correspondence, although recent work on the archive of Sraffa manuscripts at Trinity College, Cambridge indicates that the

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relation between the two is not so simple as many have thought. Some of that scholarship devoted to exploring the Sraffa archive is in this volume. There is plenty about Ricardo in it too.

There are 11 chapters plus an introduction, all 11 having been previously published, mainly in journals, and in recent years. It may be asked, therefore, whether there is any considerable purpose served by collected republication such as this? In this age of electronic availability, where the idea of 'inaccessible' publications is ceasing to have much meaning – at least for those who have access to a reasonably well-resourced library – is there any point? One thinks, in particular, of the multi-volume *Critical Assessments* collections of secondary literature on key figures in the history of economics, which were very expensively put into the marketplace over recent decades: they would surely have difficulty finding a market today, even if the Japanese market had remained so handsomely backed by 'ability to pay', as it once was. However, there is a point to such republication, if there is a coherent and substantial purpose that provides a unifying rationale for the set of articles chosen for republication, and that coherent purpose is a worthy one. These two criteria are met in the case of this volume.

In fact, the volume under review is presented by its authors in their Introduction as just one element of a larger project, embodied, so far, in this volume together with two previous collections of essays (Kurz and Salvadori 1998, 2003). What is that project? As it is stated in the prefatory advertisement to this volume, 'providing modern interpretations of the classical economists and comparing their analyses with that of contemporary mainstream economics' (p. i). Let us begin, then, by laying out the contents of all three volumes taken together. The 40 essays contained in them (11 in the 2007 volume), not including the introductory chapters, may be categorized under three subjects – the classical economists, Sraffa, and post-classical economics – although of course these are not mutually exclusive categories. Eight essays (two in the 2007 collection) focus on the classical economists as such, of which five (two in the 2007 volume) concern Ricardo; eight (four in the 2007 collection) primarily concern Sraffa's intellectual work in its own right, drawing on the authors' extensive and informed scrutiny of the Trinity archive; and 24 (five in the 2007 volume) centre upon post-classical economists and economics. As to the lack of mutual exclusiveness of these three categories, many of the essays, especially in the 2003 volume (two in the 2007 collection), deal with substantial interrelations of some kind or other between the three, throwing into sharp analytical relief the contrast between the classical and marginalist approaches.

In the volume under review, the two essays that particularly concern the interpretation of Ricardo both deal with the theory of rents. Three of the four essays on Sraffa are concerned with the making of *Production of*

Commodities by Means of Commodities (Sraffa 1960). Here, the key themes, immensely interesting, are Sraffa's intellectual activities towards establishing the theory of competitive capitalist distribution and prices on objective foundations, in the framework of circular, surplus-producing production systems, with some help from 'mathematical friends'. The further essay on Sraffa (by Gehrke and Kurz), concerning his 'difficulties' with Jacob Hollander, in relation to assembling all extant Ricardo papers for the edition Sraffa laboured on for so long, is a startling illustration of the bastardry (my term, I hasten to add, not the authors) of which 'scholars' can be capable. Recently, working on Smith's theory of economic policy, I re-read Viner's (1928: 142) comments on the pertinence of government failure, as well as market success, to understanding Smith's views – and Coase's (1976: 545) comments along somewhat similar lines: 'Politicians and government officials are also men'. By way of analogy one may say that scholars are 'men' too, and one may safely conclude that, on the basis of the compelling evidence assembled by Gehrke and Kurz, Jacob Hollander was indeed a man.

Two further essays are concerned to clarify the integrity of Ricardo's theoretical approach and its distinctness from marginalism: one on disagreements and discussions between Say and Ricardo on value and distribution, and one on Walras's criticisms of Ricardo. It must sadly be reported that the former essay does not reveal Say's intellectual abilities in a very positive light (pp. 15–19, 30–4): he could hardly have had any chance of successfully refuting a theory that, in fundamental respects, he did not even understand. The latter essay can be read as providing an early example of the proclivity of marginalist or 'neo-classical' theorists to deal with (intellectual) competition by characterizing opponents, not so much as wrong, but as particular and special cases of the 'general' marginalist theory (although Walras does claim errors in Ricardo as well; pp. 56–7). The same fate of course was to befall Keynes 60 years later. Hence the question in the title of this essay: one theory or two?

The first of the three final essays in the volume, all on post-classical economics, follows up on the authors' earlier work on the classical character of the von Neumann (1937) growth model, here formally contrasting that model also with the Arrow-Debreu short-period general equilibrium model. Kurz and Salvadori show as unsustainable the claim of Arrow and Debreu that their assumption excluding the survival problem[1] is equivalent to von Neumann's assumption of every commodity either entering or exiting all production processes (pp. 204, 214, 217–19). The following essay contrasts

1 This is the potential problem that the 'endowments' of agents in such general equilibrium models may not suffice to keep them all alive, either via direct consumption of the endowment or via trading.

Georgescu-Roegen's funds-flow approach to conceptualizing production with the classical flow-flow approach, where in the latter, fixed capital is conceptualized as a form of flow.[2] The verdict is in favour of the classical approach (pp. 235–6, 238). The final essay in the volume (by Freni, Gozzi and Salvadori) develops earlier work by Kurz, Salvadori and others on endogenous growth theory and its relation to classical approaches to growth.

There remains of course unfinished business with regard to refashioning the classical approach to economics for the purpose of understanding the contemporary mixed economy. (There is always unfinished business in a subject like economics, where the human phenomena are subject to relatively rapid historical evolution.) Perhaps the most striking aspect of this is the question of non-renewable and otherwise exhaustible natural resources. This is not a subject that is absent from the Sraffa-inspired research programme; and Kurz and Salvadori, who are evidently themselves inexhaustible sources of energy, have devoted some of their efforts to the issue also (see, for example, p. 40, n. 26; two essays in Kurz and Salvadori 1998 deal with the issue; also Steedman et al. 2001). Bertram Schefold deserves honourable mention in this regard too. But one would welcome seeing more research done in this area, given the evidently pressing practical concerns around issues of environmental sustainability today. I am struck by the fact that, in my experience, economists are much more inclined to 'technology optimism' than the general population, or even than the overall population of scientists and intellectuals. In this they perhaps share something with Adam Smith. But even Ricardo's relative pessimism (relative to Smith that is) is based upon a rather benign form of natural scarcity: the 'indestructible' powers of the earth have proved very destructible indeed; and this is by no means the major environmental problem today.

In a circular treatment of production, in which non-renewable resources enter as inputs into the production of commodities in general, the vector of surplus outputs strictly speaking must include negative quantities of those non-renewable resources. This is just an expression of the fact that a production system that uses strictly non-renewable resources in any positive quantities cannot be viable or sustainable indefinitely. This might suggest that the circular or surplus approach is not useful for the problems of environmental sustainability. I would suggest that, on the contrary, precisely *its capacity to expose the problem* might indicate that it is the right analytical tool for approaching the issue. The classical approach, framed in terms of

2 For example, a new machine enters a production cycle, in some process or industry, and leaves it at the end of that cycle as a flow of output of a one-cycle-old machine, joint with the output of the process or industry in the more usual sense (pp. 227–31).

reproduction and viability, enables the question of the sustainability of the human interaction with nature in the production of human consumption to be quite naturally posed, even if the classical economists themselves travelled not very far down the path of dealing with such issues. Whether or not technology optimism will be vindicated in the coming decades seems an open question – although one could respond that it is not so much human technological ingenuity that is at risk of hitting limits, but human morals.

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Richard N. Langlois, *The Dynamics of Industrial Capitalism. Schumpeter, Chandler, and the New Economy.* London: Routledge, 2007. 94 pp. ISBN: 0-41577-167-6

The volume is based on the 'Graz Schumpeter Lectures', given by Richard Langlois in 2004 as a part of a series of the same name, which tries to link classic Schumpeterian writings with contemporary research developments. Langlois' contribution to this series spans more than 300 years of economic theory from Adam Smith's famous 'invisible hand' to his own concept of the 'vanishing hand' (see also Langlois 2003). Metaphorically – as well as literally in an illustrating graph (p. 77) – he draws an arc between these two states of market-driven exchange that he considers to be interrupted by a mere 'episode' of managerial capitalism. However, it is this interruption, the rise and (alleged) demise of large integrated corporations guided by the 'visible hand' (Chandler 1977) of managers, to which Langlois devotes his attention.

From the more general claim that organizational change or the evolution of 'social technologies' (Nelson and Sampat 2000) can be factors in economic growth, he derives what he calls the 'Schumpeter–Chandler thesis'; namely that the rise of large corporations was a driver of economic growth. This thesis may not be provocative for institutional economists from Thorstein Veblen to Douglas North but very well challenges traditional and post-war 'neoclassical' approaches towards explaining economic growth and development just via aggregates of capital and labour, leaving technological and organizational developments aside by assigning them a purely exogenous role.

Langlois himself, however, immediately contrasts his own thesis with two contradictory claims. First, the importance of charismatic entrepreneurs for innovation and economic development, whose logic of individually driven creative destruction is very different from large and 'rationalized' machine bureaucracies; a dichotomy that can be found already in Schumpeter's writings. But on his way through an obligatory exegesis of Schumpeter's most prominent volumes (Schumpeter 1934, 1950), he vehemently rejects the common distinction between 'two Schumpeters' (see for example Fagerberg 2006) and attributes differences in his writings not to changes in opinion but to changes in the subject (capitalism) as well as to the coexistence of two inconsistent epistemic approaches in his works: rationalist and empiricist theories.

In interpreting the Schumpeterian notion of entrepreneurship as a correspondent to Weber's concept of charismatic leadership, Langlois delineates the entrepreneur as both agent and victim of a 'transformation from the traditional to the rational'. The 'animal spirits' of a charismatic entrepreneur are necessary to creatively destroy traditional structures but at the same time they build new structures that tend to make them dispensable. A point he illustrates with the example of Nicolas Hayek, the 'Messiah' that saved the starving Swiss watch industry by founding the Société Suisse de microélectronique et d'horlogerie in a process of 'progressive rationalization'.

As a second contradiction to the 'Schumpeter–Chandler thesis' Langlois mentions recent trends of 'de-verticalization', coming up with the question of the remaining contribution of Schumpeter and Chandler to economic growth theory. In asking the question 'Why did ''managerial capitalism'' supersede ''market capitalism'' [...] in the late nineteenth century?' (p. 9), he not only reveals his own background as a transaction cost economist inspired by Coase, he also refers to both his answer and his current theoretical camp: in embedding transaction cost arguments in an evolutionary framework of economic development, he describes managerial capitalism as a temporary answer to 'an evolutionary design problem' (p. 11). So, similar to his explanation of inconsistencies in Schumpeterian theory, he then somehow reconciles at first glance incommensurable approaches in organization theory such as 'resource dependence' perspectives (for example, Pfeffer and Salancik 1978) and 'resource based' views (Hamel and Prahalad 1994) – explicitly, however, he only refers to the latter. For him both can be valid explanations for dominant forms of organizational design contingent on complementary historical developments in terms of institutions (e.g. liberalization of international trade) or technologies (e.g. transportation and communication costs).

Generally speaking, Langlois paints a very rich theoretical picture, citing the 'who is who' of (at least: institutionally inspired) economic theory as supporters on his way from the 'invisible' to the 'vanishing hand'. One major flaw, however, remains: although he obviously tried to deliver a coherent volume, it is undeniable that each of the five chapters roots in originally independent articles. In other words, the book is a lively written and interesting-to-read synopsis of Langlois' works but neither is it completely harmonious nor does it contain really new insights for anyone who already knows his regular articles.

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Fransisco Louçã, *The Years of High Econometrics. A Short History of the Generation that Reinvented Economics.* London: Routledge, 2007. xxix + 370 pp. US$180.00. ISBN: 978-0-415-41974-1

Fransisco Louçã, Professor of economics at the ISEG (Instituto Superior de Economia e Gestão) in Lisbon, has written a fascinating book on the history of econometrics. It is fascinating as the book describes many interesting episodes of the early history of econometrics, mainly during the interwar period. The episodes are presented with the support of extensive amounts of unpublished archive materials, including lecture notes and correspondence of various kinds. In comparison with earlier books on the subject, such as Morgan (1989), this book is particularly informational on the historical background of how economics has been reinvented through the establishment of the Econometric Society and the journal *Econometrica.* The author is to be congratulated for assembling all the materials together and providing us with such an interesting and enjoyable read, especially in contrast to the dry, technical and ahistorical nature of most of the econometrics books available in the present academic circle.

Organized into four parts and 12 chapters, the book covers the foundation and construction of econometrics, as well as well-known debates and other issues at the edge. Ragnar Frisch is placed at the centre as far as the substance of the book is concerned. Particularly valuable are the descriptions of two sets of Frisch's unpublished lecture notes: one delivered at Yale University in 1930, and the other at the Poincaré Institute in Paris in 1933.[1] These lecture notes give a rare exhibition of Frisch's overview on economics and econometrics in an accessible but astute manner. These notes will help deepen our understanding and appreciation of Frisch's pivotal contribution to the formation of econometrics substantially.

Louçã's book will not only help readers of none econometrics professions to learn and understand the history of econometrics, but will also encourage readers of the profession to reflect critically on the methodological development of the subject and, hopefully, on the direction and the objectives of their own research. Several issues covered by the book bear great significance to the way that econometrics has been evolving up to its present day. For instance, the concerns are still valid over the closeness that an analogy could be drawn between economics and physics, and over the extent to which mathematics and mathematical statistics could really sharpen and improve economic analyses, although mainstream economics of today has been virtually mathematicalized.

1 The Poincaré lectures have been translated into English by O. Bjerkholt and A. Dupont-Kieffer, and are to be published as a monograph by Routledge.

Methodological debates among the earlier generation of leading economists serve as a high reminder that it is *non sequitur* to equate heavy use of mathematics with scientific value and practical fruitfulness. Noticeably in the last part of the book, Louçã describes the social and political ideologies held by those major players. He maintains that the serious social responsibility assumed by these players exerts great impetus to their econometric research, which shaped the growth modern economics. This part of the book suggests the author's disapproval of those research works by economists and econometricians of the later generation who show little interest in the real world and are deeply lost in the thick mist of formal mathematics twiddling. In fact, the book concludes with a short description of how some of the early founders felt disappointed at the development of econometrics during the 1950s and the 1960s, as it moved away from embodying social responsibility to become what Frisch referred to as 'playomatrics'.

Louçã's critical stance on econometrics is commendable. However, the book still leaves much to be desired from an econometrician's viewpoint. Taken as a whole, the book is biased towards the mathematical side of the developments. The history depicted in there stays somewhat apart from what econometrics is commonly understood nowadays. Little is covered about data analyses and empirical studies, whereas much is discussed on model formulation. The book reads more like a history of macroeconomics than that of econometrics by the present demarcation of subdisciplines within economics. There are also omissions and inaccurate interpretations in a few places. One important omission from the list of characters is H. O. A. Wold. Wold's contribution to econometrics actually well exceeds the majority of the characters in the list, either by his pioneering endorsement of probability theory ahead of Haavelmo (see Wold 1938), in spite of Frisch's distaste (see Hendry and Morgan 1994), or by his later opposition against the Cowles Commission simultaneous-equations modelling approach (for example, see Wold 1954). Without the contrast of Wold's causal chain time-series approach, the Cowles Commission programme is depicted as one of generalizing the stimulus-response approach explored by Frisch (see Chapter Four). That is, unfortunately, a gross misinterpretation of the Cowles Commission's contribution, as known to be embodied in Koopmans (1950). Modelling issues on simultaneity and static equilibrium dominated overwhelmingly those of dynamics during the Cowles Commission era. A reversal of the situation did not occur until about three decades later, marked by Sims' (1980) strong advocacy for the Vector AutoRegression approach. In fact, the history of the stimulus-response modelling approach is a labyrinth. Chapter Six discusses it again around the works of three characters: Slutsky, Frisch and Lucas. However, the discussion there fails to clarify what has led Frisch and Slutsky to their opposing views – the mathematical equivalence between autoregression (AR) and moving-average (MA) models. While Frisch chose the AR scheme to describe economic structures, Slutsky extended his interpretation of the economic world based on an MA representation. The choice entails making implicitly a crucial assumption about the economic property of the error term; that is, whether the error term is model derived or independent and identifiable as a particular structural shock (for example, see Qin and Gilbert 2001). Many economists and econometricians are confused or ambiguous about that point. The book misses that point too in Chapter Six as well as in Chapter Eight, where the nature of randomness is discussed in relation to probability concepts.

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Leonidas Montes and Eric Schliesser (eds), Foreword by Knud Haakonssen. *New Voices on Adam Smith.* London and New York: Routledge Studies in the History of Economics, 2006. xxi + 364 pp. £80.00. ISBN 0-415-35696-2.

Taking the general resurgence of interest in Adam Smith into consideration, the editors of the volume under review put together original, commissioned, and refereed papers by scholars who defended doctoral theses on Adam Smith or closely related topics between 2000 and 2004. As they declare in their introduction, selection criteria included not only quality and originality, but also disciplinary and geographic diversity. Economists are a minority amongst the interdisciplinary group of contributors, including scholars in the fields of Political Science, Social Theory, Philosophy and Literature. The portfolio of selection principles chosen by the editors has interesting side consequences: taken as a whole, the collection illustrates the emergence of a kind of *Adam Smith Studies* (culminating in the newly founded Adam Smith Review) within which the ongoing difference in interpretative frameworks is *not primarily* triggered by disciplinary boundaries.

The volume is subdivided into four parts. Part I includes contributions by Ryan Patrick Hanley (on Adam Smith, Aristotle and virtue ethics), Edith Kuiper (on Adam Smith and his feminist contemporaries, whose intellectual relations to Smith turn out to be pretty remote) and Robert Mitchell (on the role of systems in Smith's 'aesthetics of political improvement'). The general title of part I – in view of the content of these papers, not entirely compelling – is 'Adam Smith, his sources and influence'. In a very specific and interesting sense, the issue of 'influence' is dealt with by Mitchell: his paper does not so much address questions like 'Which are the main intellectual resources used by Smith, and how did he use them?' It is also not so much about some details of the multi-faceted stories on Smith's own influence upon others (even though his importance for Burke, Godwin and Coleridge is discussed and analysed). Mitchell's main concern is Smith's *general reasoning* concerning the *systematic* conditions and implications of influence of intellectual edifices in the real world, influence that, according to Smith, tends to mediated by 'love of system'. Put another way, Mitchell's well-informed paper addresses the important topic of the ambiguities and the functions of *systems* within Smith's reasoning. He discusses Smith's careful and sophisticated reflections on the role of systems and the love of system, most notably in 'Theory of Moral Sentiments' (Smith 1976b) and 'The History of Astronomy' (Smith 1980). This prepares the ground for a potentially revealing contrast: Smith's critical reflections on systems in 'Theory of Moral Sentiments' (Smith 1976b) and 'Essays on Philosophical Subjects' (Smith 1980) versus the far-reaching influence of the author of 'An Inquiry into the Nature and Causes of the Wealth of Nations' (Smith 1976a) and his own *system* of natural liberty. Issues related to this problem are discussed in one of the best papers of the volume by Lauren Brubaker. He analyses Smith's view of the systematic role and scope of economic policy, in particular in bringing about the institutional pattern that is required for *the system of natural liberty* introduced in 'An Inquiry into the Nature and Causes of the Wealth of Nations' (Smith 1976a). Lauren Brubaker's article 'Does the wisdom of nature need help' is worth reading for two reasons: first, Brubaker identifies the problem in a very crisp and clear way; and second, he comes up with interesting ideas how to address it.

Brubaker's article is the penultimate contribution to Part II, which includes four other pieces on 'Adam Smith and moral theory'. Fonna Forman-Barzilai discusses Smith's reasoning on 'connexion', culture and judgement, while a paper by Carola von Villiez deals with Smith as a forerunner of Rawls's reflective equilibrium (a concept which of course has further and more distant forerunners, e.g. in classical antiquity). In both papers, Smithian reasoning is confronted with the much-debated issue of tension between moral/cultural pluralism and the ethical justification or the status and function of universal norms. 'Smithian environmental virtue ethics' is discussed by Patrick Frierson, while Chad Flanders provides a substantial and subtle attempt to make sense of the well-known passages in 'Theory of Moral Sentiments' (Smith 1976b) that invoke the evaluative role of actual consequences of actions in a probabilistic environment.

Part III is devoted to 'Adam Smith and economics'. Meta-economical issues (such as Leonidas Montes's discussion of Smith's Newtonianism and general economic equilibrium theory) loom large also in this part. All three authors of this part (Maria Pia Paganelli writes on vanity and paper money, Jimena Hurtado-Prieto on Smith's comments on Mandeville) obtained their Ph.D. degrees from economics departments. The volume concludes with three essays on Smith and knowledge: Craig Smith writes on 'Adam Smith on progress and knowledge', Estrella Trincado on the utilitarian theme of the 'creative present', and Eric Schliesser on Smith's benevolent and self-interested conception of philosophy.

It is obvious that in a short review it is impossible to do justice to all of these contributions, all of which touch (and some of which document some progress with respect to) the analysis of deep and complex issues. While the new interest concerning Smith the philosopher and the concomitant widening of the scope of the interpretive frameworks promises gains in various respects – amongst them also those paying off in terms of a better understanding of Smith, the political economist and of Smith, the outstanding figure of economic liberalism – some of the papers provoke a kind of uneasiness. To be sure, it is entirely legitimate to pursue the question of an Aristotelian heritage or proto-Kantian themes in Smith. Nonetheless, I believe that the value of some discussions would be enhanced by putting them more explicitly into the perspective of some main coordinates pertinent for the understanding of Smith. Let me mention two of such coordinates:

(1) Smith seeks to establish ethics on the basis of moral sentiments after Hutcheson and Hume, whilst some of his British contemporaries (whose concerns about the gloomy implications of the 'fatherless world' conceptualized by Hume and others is shared by Smith) turn from the *sentimentalist* tradition to a *rationalist* foundation of ethics.

(2) In a tradition inaugurated by Grotius and invigorated by Hume, Smith emphasizes the pivotal institutional role of justice that is the only virtue with respect to which *precise* judgements can be made and with respect to which further scientific elaborations of the problems are promising.

Various enquiries concerning Smith's sources, concepts and stances would be more meaningful when account is taken of suchlike fundamental coordinates. An example in case is the relation between Smith and prominent protagonists of virtue ethics. For Smith, norms of justice are clear-cut constraints – whereas with respect to other ethical norms moral learning and balancing is (as emphasized in 'Theory of Moral Sentiments'; Smith 1976b) complex in a way that may be said to have some commonalties with Aristotle's view. One is in a better position to show how Smith preserved 'desirable aspects of ancient thought within modernity' (Ryan Hanley quoting C. Griswold on p. 33) if one states the differences in the problem settings as well as with respect to some key features of the proposed systems as clear as possible at the outset.

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D. P. O'Brien, *The Development of Monetary Economics – A Modern Perspective on Monetary Controversies*. Cheltenham/Northampton: Elgar, 2007. 265 pp. ISBN 9-781847-202604

'The literature of monetary economics is perhaps the oldest part of the literature of economics as a whole' (p. 1).[1] This introductory statement of

1 All page references are to the O'Brien book under review.

the author, Emeritus Professor at the University of Durham, and the main title of book let the reader expect an all-embracing, voluminous study; but an inspection of the table of contents shows that the chapters, from which some were already published elsewhere, after reviewing Bodin, Law and Locke, concentrate on nineteenth-century debates in Britain. Thus the programme is well contained, and the announced 'modern perspective' most clearly shows up in the final chapters' attempt to translate the views of Bagehot and Joplin into the language of formal macroeconomic models.

After the introductory Chapter One, O'Brien argues in Chapter Two – despite some critical assessments in previous debates – for regarding Jean Bodin as the pioneer formulator of the quantity theory of money. This theory is summarized as consisting of a money demand that can be envisaged as being independent of money supply; a money market clearing assumption where the adjustment process basically entails movements of the goods market price level; and the statement that causality runs from money supply to prices. Bodin counts the abundance of gold and silver as one main cause for rising prices, but, besides debasement, also listed luxury demand, trade-induced scarcity and even monopolies. Bodin deserves respect for his doubt that the last-mentioned item really can drive true inflation; and, as opposed to the general mercantilist thinking, he also had an understanding for the wealth-creating effects of trade. His views on the determinants of money demand and velocity remain somewhat vague, however. Reviewing the work of Bodin helps to see how slowly economists at that time improved their understanding of absolute (i.e. money prices) compared with relative price changes. O'Brien presents the Salamanca-School member Navarrus who, writing somewhat earlier than Bodin, argued that the laws of relative scarcities applied to all commodities, including money, so that its relative abundance compared with other goods drives its relative price. It was still a long way to go before money as fiat money is being regarded not just like any other good, but a specific means of payment standing opposed to the bundle of goods.

Chapter Three is on John Law's famous treatise 'Money and Trade Considered'. Approaching the work of this mercantilist writer from Keynes's view on the matter, the reader discovers that both appear to disagree on an important case in point: it is well known that Keynes (1936: 336) defended the mercantilist striving for export surpluses as a rational strategy for enlarging the domestic money supply by way of attracting foreign reserves, in a time when the society still did not dispose of a central bank that could be charged with the task of supporting economic development. Following O'Brien's concise and illuminating presentation, the reader learns that Law (mainly in Chapter II of 'Money and Trade') develops a line of argument that turns the sequence of events around: 'A balance of payments surplus, by increasing the money supply, would stimulate exports' (p. 39). Obviously, money funds were seen as a supply constraint of employment and production. Therefore financiers and creditors ranked high in mercantilist theory. The focus is on the supply-side aspect of finance, not Keynes's message that from a macroeconomic perspective income is created by monetary effective demand. Thus the mercantilists' understanding of the working of a monetary economy led to a kind of monetary supply-side policy. Law, however, gives no argument why an increased production would necessarily be sold abroad; also he does not mention that imports might rise in line with domestic production and income. Perhaps the complementary relation between the 'Keynesian' and the 'original' logic of mercantilism could have been mentioned and worked out to some extent in the book.

Although the orthodox history of economic thought presents Law as an 'early' economist with faulty and politically dangerous views, the reader learns from O'Brien that Law praises money for its service of overcoming the inconveniences of barter, just like the classics, and he cannot be blamed for stressing the importance of finance for a developing economy. Surely he describes credit in money terms, and he is right to do so when analysing a monetary economy. The possible accusation of succumbing to a kind of money illusion is not convincing, as Law understands the crucial condition of price stability; only 'real money supply' contributes to economic development (p. 41). However, O'Brien is rather generous not to focus the conspicuous confusion between the notions of money and credit in mercantilist writings of Law and Locke. He summarizes the opinion of the latter by saying that 'the money supply is viewed not in terms of bank credit [...] but in terms of specie' (p. 61). Despite the Post-Keynesian assertion that all money is credit,[2] it is inappropriate to muddle different items of banks' balance sheets. Factually, this was the basic flaw of the early one-tier banking systems that the extension of credit at the same time enlarged the stock of money. Law stated that fact explicitly,[3] but was unable to see the roots of monetary instability.

What he also did not see – and O'Brien emphasizes this clearly by demonstrating Law's logic with the help of a formal model – is that the level of prices, particularly of assets like real estate, is undetermined if the banks issue money on demand on the security of land pledged. The very fact that makes land appropriate for serving as a security in the eyes of Law, namely that it is fixed in supply, causes its price to explode in the course of a

2 'Loans constitute the majority of money' (Arestis and Eichner 1988: 1017).
3 'The more the banks lend out, the more they increase the amount of money' (Law 1705: chapter 3).

bubble. Looking at Japan in the late 1980s reveals that even modern economies with a two-tiered banking system may suffer from similar instabilities.

Chapter Four portrays, first, John Locke – who, although in favour of low interest rates like practically all mercantilists, is to praise for his understanding that low rates cannot simply be stipulated by monetary authorities. Interest rates were already low in Holland as a *consequence* of the high financial and economic maturity of that country; thus low rates mirror low risks and high reputation, features that have to be *earned*. The little known Joseph Massie is introduced, second, as an early herald of the classical belief that interest rates depend on the rate of profit. Like Adam Smith, he expected a secular decline of profit as a consequence of increased competition at home and abroad. O'Brien attacks Massie's view mainly on empirical grounds: price calculations are said to be defective; interest rates remained steady, contrary to prediction; and 'claims about the level of "the" profit rate could have little basis in fact' (p. 73). Any reader adhering to the bare logic of a classical model, however, will find this attempt of playing off empirical impressions against analytical reasoning as less convincing.

This chapter also contains some discussion of balance-of-payments aspects of monetary expansions. Contrary to Locke, David Hume is known for his insights into equilibrating goods and specie flows between countries. O'Brien devotes not much space to this topic, which surely could have been explored more deeply because the self-adjusting mechanism in Hume's approach cannot be taken for granted. Contrary to his prediction, national price level would fluctuate in an anti-cyclical manner, they moved more or less in line, hinting to the influence of a 'key country's' monetary policy that acted as the 'conductor of the international orchestra' (Keynes 1930: 306–7). In Hume's model, deficit countries should not bother as gold outflows would induce price increases abroad, and would automatically correct a trade deficit; actually, these countries were forced to run restrictive monetary policies in order to avoid distress in the national banking system.[4]

After a clear presentation of classical trade theory and the purchasing power principle, Chapter Five starts a review of the famous Currency-Banking controversy. Whereas classical economic theory is often believed to imply 'passive' economic policies, the reader learns from O'Brien that things were different. He quotes Bank of England Director Norman who expresses the strong market signals and incentives for a (central) banker to

4 More on this asymmetric tendency in the era of the classical gold standard can be found in Spahn (2001).

behave pro-cyclically (p. 97). Some kind of monetary control was needed, and neither the Banking nor the Currency School had a sufficient understanding of that task. This is rather obvious with regard to the first-mentioned School that failed to draw a clear demarcation between money and credit.[5] If money supply follows the 'needs of trade', any harvest or shock-driven cycle will be amplified, falsifying widespread beliefs that prices were tied down by real, long-run cost determinants. For adherents of the Currency School, who were to be met among practical (central) bankers, it appeared obvious to limit the amount of note issue, although they recognized that this would only help if other means of payment, country bank notes in particular, would vary accordingly; thus the Currency School hopefully came to believe in a money multiplier.

Chapter Six gives a rich report of the learning process on the part of the Bank of England of how to overcome the practical difficulties of monetary control. The key event was the separation of the Bank of England into the Issue and the Banking Department, of which the former was charged with the macroeconomic task of controlling the quantity of money (i.e. notes) on the basis of a strict gold-backing rule, whereas the latter was allowed to orientate its credit supply according to the profit motive. O'Brien presents persuading econometric evidence for his claim that three aims of monetary policy reform could not be reached: the level of the Bank's bullion holding, prices and interest rates fluctuated as before. Moreover, the banking system was hit by severe liquidity crises, a not at all surprising by-product of a policy of strict money supply control. O'Brien touches upon this topic in passing. Bagehot would notice later: 'Any notion that money is not to be had, or that it may not to be had at any price, only raises alarm to panic and enhances panic to madness' (1873: 28) – in other words, a quantitative constraint of money supply destabilizes money demand. This is why monetarist policies like Friedman's *k*% rule never worked, and were practised only for short period of time after the 1970s. O'Brien abstains from elaborating on this issue, surely a legitimate decision as his book concentrates on nineteenth-century debates. However, some more information could have been provided on the historical irony that it was the Banking, and not the Issue Department, which over the years learnt to take over the role of the country's central bank by engaging in active interest rate management.[6]

5 O'Brien remarks that some Banking School writers stretched the concept of money 'to an almost Radcliffian vagueness' (p. 104). Interestingly enough, after some decades of financial innovation, official M3 definitions that are used also in 'monetarist' central banks come close to the Radcliffian notion of liquidity.

6 It should be noted that the use of the interest rate instrument in the beginning was meant to serve the profit motive of the Bank. Macroeconomic stabilization ensued as a by-product (cf. Ziegler 1990, Spahn 2001: chapter 4.5).

The liquidity problems within the banking system put the lender-of-last-resort issue on the agenda. This is discussed in Chapter Seven. Already Thornton had shown that providing distress refinancing to banks would not jeopardize monetary control. But the more important argument most probably was that a failure of commercial banks also might pose a threat for the reserve of the Bank of England. Its directors were split over the issue, however. Director Norman made the proposal that the Bank should sell off (!) government debt in order to increase the scope for enlarged distress lending – which implied that he had not grasped the essence of a liquidity crisis. And after 1844, as O'Brien rightly says (p. 171), 'the state was set to disaster' because Peel's Act counter-intentionally had abolished the slowly grown roots of central banking; the Banking Department was happy to re-enter the money market as a competitor, rather than as an emergency agent, of other private banks. Therefore, monetary authorities had to re-learn already forgotten lessons. The banker Thomas Joplin was an important figure in the development of the lender-of-last-resort doctrine, as was Bagehot who added to the points already made by Thornton, what O'Brien calls, the suggestion of a penalty interest rate for controlling the access to emergency lending.[7]

The last part of O'Brien's book, Chapters 8–10, is devoted to the innovative attempt of translating the considerations of Bagehot, Joplin, and Currency-Banking followers into formal macroeconomic models. This no doubt is a demanding job, as the writings of these economists sometimes appear to lack analytical rigour. Collecting their assumptions and verbal argumentations, O'Brien builds rich, non-linear models that provide a useful completion of the analysis in the book's previous chapters. The elaboration on Bagehot is particularly interesting; in a way his monetary policy prescription can be seen as a precursor of a Taylor rule where the central bank interest rate reacts to macroeconomic imbalances.

Seen as a whole, O'Brien's study does not constitute a comprehensive survey, but the selection of authors and topics offers an easy-to-read view on important monetary debates in pre-classical and classical economics. Even if the reader here and there might miss some hints to modern debates and monetary control problems, the book is most valuable for the study of early monetary theory and policy.

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7 Goodhart (1999) challenges this widely held interpretation of Bagehot's suggestion and argues that the stipulated rate could well be at the contemporaneous level; the word 'penalty' rate would not appear in *Lombard Street*.

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Vilfredo Pareto, in Roberto Marchionatti and Fiorenzo Mornati (eds), *Considerations on the Fundamental Principles of Pure Political Economy.* London: Routledge, 2007. XXX + 162 pp. £70.00 (hardback). ISBN 13: 9780415399197.

This volume collects the five essays published by Pareto on *Giornale degli Economisti* in 1892/93, under the original title *Considerazioni sui Principii Fondamentali dell'Economia Politica Pura,* for the first time translated into English. The translation is very accurate. The introduction by the editors clearly and precisely illustrates the genesis of these articles and Pareto's position in relation to the economic literature of his time. The notes of the editors to Pareto's text are primarily historical in nature, and exhaustive from that point of view. Only a few notes, without pretensions of systematicity, have an analytical nature and serve to explain Pareto's mathematical reasoning.

These articles made Pareto appreciated, among others, by Edgeworth and Walras, and led Walras to propose Pareto as his successor in Lausanne (where Pareto started to teach in mid-1893).

The content of these articles is partly methodological and reflects Mill's approach to scientific logic. In this respect, Pareto's position is intermediate with respect to Walras and Marshall. He is distinct from Walras (and thus closer to Marshall) in so far as he assigns to pure political economy the task of representing, albeit in a schematic way, and rationalizing observable economic reality, versus Walras' abstract pure rationalism. But he is distinct from Marshall (and closer to Walras) because he does not consider the assumption that marginal utility of money is constant a good approximation of reality. This assumption enables partial equilibrium analysis and does away with the analysis of market interdependencies. For Pareto, it is these very interdependencies that justify the extensive recourse to mathematics and, without it, a satisfactory analysis of market equilibrium with a plurality of goods would be impossible.

In this phase of his life, Pareto is, epistemologically speaking, a positivist. As he will emphatically note in the opening pages of the *Cours*, economics 'is a natural science just like psychology, physiology, chemistry, etc'. Even though in the third of the essays he lets out a reference (p. 76) to 'moral and economic sciences', which would suggest a differentiation between natural and moral sciences, with economics closer to or included in the latter, Pareto's position in this respect is very different from that of the Cambridge School (from Marshall to Keynes), which insisted on the moral nature of economic science. For Pareto, the study of human society is not methodologically distinct from the study of animal societies.

However, the main subject of the essays is demand theory. Production is absent, but it will be examined and innovatively treated by Pareto in later writings (including the 1894 article where he introduces the analysis of a Pareto optimum, and his *Cours*, in 1896/97). The analysis of individual choice is explored in significant detail and contains elements that will be developed in the *Manuel*.

In the remainder of this review, I will limit myself to consider the following two problems: the determination of the dependency of demand from prices (i.e. those relations that today go under Slutsky's name), and the determination of total utility from individual choice (today summarized by the category of integrability conditions).

Derivatives of demand functions with respect to prices are presented in the third essay in reference to the case in which marginal utilities are simply functions of the quantity of the good in question (the case that corresponds to a total utility function exhibiting additive separability), and, in the fifth essay, in reference to the general case in which marginal utilities are functions of the quantities of all goods. For both cases, Pareto determines the derivatives of demand functions. In the first case, he shows that these imply that, if marginal utility functions are decreasing, then demand for every good is a decreasing function of the price of the same good and an increasing function of the price of the other goods, which means that, to use today's language, all goods are gross substitutes and there are no Giffen goods. The derivatives for the general case (like the more elaborate ones listed in the appendix of the *Manuel*) are so similar to those developed by Slutsky (1915) that there has been significant debate on whether Pareto anticipated Slutsky's analysis (see, among others, Schultz 1935, Dooley 1983).

The determinability of utility from choices (largely corresponding to what in contemporary theory goes under the name of revealed preferences, which include the issue of integrability) is an important subject for Pareto, who always seeks to link theory to reality. There are many references to this problem in these essays, but there is not a systematic treatment. A mature version will be proposed in the appendix of the *Manuel*, where the empirical starting point is constituted by marginal rates of substitution. In these essays the starting point is constituted by marginal utilities (which are represented by inverse demand functions). Already in the first essay, Pareto asks whether in the analysis of individual choice one should start from a total utility function or from marginal utility functions, opting for the latter. The problem is that while the individual is often able to compare changes in utility determined by small changes in consumption, the same cannot be said for big changes, and also that marginal utility functions do not always allow for the existence of a total utility function of which they are the derivatives. The issue is discussed at length in the fifth essay, where he connects the existence of the total utility function to the independence from the order of consumption (in other works, the hypothesis that the vectorial field constituted by marginal utilities is conservative, thereby allowing a scalar field, represented by total utility, of which it is the gradient). Pareto not only notes that the existence of the total utility function is subordinated to integrability conditions if there are more than two goods (something he forgets about in the *Manuale*, thus inviting Volterra's critical remark), but also that total utility is defined save for an arbitrary monotonic transformation. However, shortly afterwards he writes down mistaken integrability conditions for the case with three goods. (Yet he had had the chance of looking at Antonelli's essay published a few years before, which presented the correct integrability conditions, as well as Irving Fisher's *Investigations* that are cited).

These essays highlight the innovative character of Pareto's analysis in the context of the Marshallian–Walrasian neoclassical approach. In them, Pareto introduces original ideas and delves deeply into analyses that will be the foundations for his mature treatises on the subject. Their presentation in English is extremely useful for all scholars studying Pareto and the formation of the theoretical system of mainstream economics.

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Vilfredo Pareto, in Aldo Montesano, Alberto Zanni and Luigino Bruni (eds), *Manuale di economia politica*. Milano, Università Bocconi Editore, 2006. 706 pp. €45.00.

For the first critical edition of Pareto's *Manuale* (printed at the end of 1905 but with 1906 given as its publication year) the editors chose to add the Italian translation of all the novelties that Pareto put in the second (French) edition. It was undoubtedly a happy choice, because it allows one to read both the *Manuale* and the *Manuel* in a compact form. But, according to us, it would have been much easier for the reader to consult the valuable comments by the editors in footnotes rather than at the end of the volume. These comments are by two well-known paretian scholars: Aldo Montesano and Alberto Zanni.

Zanni's notes explain the text and, most of all, discuss deeply several questions pertaining to Pareto's thought that were aroused in Zanni by his reading of the *Manuale*. They are all interesting because they contain a wealth of valuable bibliographical notices and analytical interpretations that are of course open to fruitful discussion. With regard to note 2, for instance, we do not believe that there is too much stress given to the fact that the theoretical difference between Pareto and Marshall lies precisely in the former's adhering to the general equilibrium model, and the latter's to that of partial equilibrium.

Montesano's notes are appended to the Italian translation of the French mathematical appendix, of which they constitute an original advanced mathematics interpretation whose appreciation is particularly challenging for the reader (if not a mathematical economist). The effort is, however, abundantly rewarded in terms of the comprehension of many convoluted (and occasionally incorrect) formal passages by Pareto. As a simple example we shall mention notes 10–24, which make it possible to appreciate the implications of the well-known question of the problematic determination of the indifference function starting from the integration of a differential equation with more than two goods. Not least among the credits of Montesano's notes is the correction of Pareto's many misprints and oversights, which contribute to make the appendix exceedingly difficult to understand.

If all these notes will be reprinted into a monograph, this latter shall quickly become classics of Pareto scholarship.

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Rick Tilman, *Thorstein Veblen and the Enrichment of Evolutionary Naturalism.* Columbia, MI: University of Missouri Press, 2007. xxii + 344 pp. $30.66. ISBN 978-0-8262-1714-1.

Over his career, Rick Tilman has written a vast amount on the ideas of Thorstein Veblen in the form of both articles and books. There are noticeable themes in this body of work. Tilman has always tended to focus on the political and sociological in Veblen's work, rather than on his economics solely; he has worked at placing Veblen in the context of other major sociologists such as Durkheim, Mannheim, Parsons, and C. Wright Mills, among others; and he has paid attention to aspects of Veblen's work that have been relatively neglected by others—Veblen's views on religion and his aesthetics are just two examples. This book carries these themes forward, but in a way that attempts to link Veblen's work to a tradition of 'evolutionary naturalism' and to argue for his having had a key role in 'enriching' that tradition, both in terms of its intellectual scope and in terms of developing it in ways consistent with 'progressive social change' (p. 10).

Tilman presents Veblen as an evolutionist, a naturalist, a secular humanist, and as a left progressive. There can be little doubt of Veblen's evolutionary credentials. Tilman shies away for exploring the current debates surrounding whether Veblen's views on cultural evolution can be seen as a generalized version of Darwinism or not, but the precise nature of Veblen's evolutionism is less important here than his insistence on cumulative causation driven by an ongoing interaction between the material means of life and the broader institutional framework. Evolutionism and naturalism have obvious compatibility to the extent that both reject reference to the supernatural in the explanation of the world and of the human race and culture. There is no doubt that Veblen saw the fundamental aspects of human nature as deriving from natural processes of evolution and natural selection. Supernaturalism is seen by Veblen as itself the product of a naturally acquired instinct – the instinct of idle curiosity. Idle curiosity produces explanations of the universe, whether of the form of myth, religion, or science. Veblen's position, however, is that the systems of thought idle curiosity produces, *including science*, all necessarily contain metaphysical premises. When applied to science, then, Veblen's naturalism has its limits.

A not entirely dissimilar point can be made in connection with Tilman's discussion of Veblen's treatment of values. Tilman is perfectly aware that Veblen tended to reference issues of value back to what he called the 'generic ends of life', which in turn relate primarily to the instincts of workmanship and parental bent. This implies some kind of 'value constants' at least at a high level of generality. But why such instinctive ends should be given such a special status when they are themselves the products of blind cumulative causation and natural selection, which Veblen did not usually equate with normatively desirable outcomes, is far from clear. Tilman, as others, equates these values with human survival, but Veblen's own analysis of human history indicates that cultures based on emulation and status have dominated ever since the end of the prehistoric 'savage' era. Maybe some degree of emulation is not necessarily so bad.

There is also, in Veblen, a failure to consider the problem of the resolution of possible conflicts between the basic values of workmanship (efficiency) and parental bent (equity). Tilman wants to overcome such problems by squeezing Veblen's work on values into an instrumentalist mould provided by John Dewey. Tilman, of course, is far from alone in this attempt to reconfigure Veblen's writings into a more amenable shape, but the effort tends to founder on the lack of any clear instrumental argumentation in Veblen's discussion of how values are formed, reconciled, or changed over time. Instrumentalism is a philosophy based on the appraisal of consequences. For Veblen, values are habits of thought that emerge from the underlying patterns of life, and changes in values arise from efficient causes and not from sufficient reasons. Related to this is Tilman's recognition that Veblen was no progressive reformer of the John Dewey type, and had little faith in 'efficacy of positive government' to bring about desirable social change. Dewey's analogy between science and democratic discourse is nowhere to be found in Veblen. Tilman goes on to link up Veblen's evolutionary naturalism with 'secular humanism' as defined by Paul Kurtz (p. 34). Veblen surely did accept some type of secular humanism, but there are issues with the exact the nature of this humanism – issues that become more apparent later in the book.

Tilman's idea of Veblen as evolutionary naturalist and secular humanist is then used as a basis for the discussion of numerous other themes in Veblen. Veblen's evolutionary naturalism and secular humanism are seen as underlying and providing unity to all of Veblen's work. These discussions range over an extremely wide set of topics, including Veblen's theory of cultural change and 'cultural lag', his views on sport, religion, and gambling, his aesthetics, his 'sociology of control', the biological aspects of his work, his sociology of knowledge, and the relationship between his work and structural–functional theory in anthropology. All of this is supposed to show how Veblen 'transformed' evolutionary naturalism into social theory and social criticism and 'progressively enriched' both economics and sociology: 'progressive' in both the sense of 'broadening and deepening the channels of economic and sociological inquiry' and in the 'politico-cultural sense of the values of the left-liberal side of the ideological spectrum' (p. 297).

There is of course a point to Tilman's argument, but it does have to be remembered that Veblen was far from the only American social scientist pursuing evolutionary and naturalistic ideas at the time, and far from the only one adopting progressive political ideas. Veblen's ideas did have an impact on American sociology and economics, most notably in terms of his discussion of consumption and the leisure class (in sociology), and in terms of his critical view of business and the inadequacy of markets as a method for the social control of business (in economics). But it would be difficult to argue that Veblen's specific theories of social evolution, or his views on many of the other topics covered in Tilman's book, had that wide an impact.

There are also particular problems with Veblen's work in many areas, from his notions of the 'discipline' of the machine process (Rutherford 1998), to his aesthetics with its strong emphasis on the efficient and the useful. Tilman argues that Veblen can be seen as attempting to provide a non-invidious aesthetic for the common man (p. 143), but the best example Tilman can provide turns out to be the art of Winslow Homer. If Veblen's aesthetics cannot encompass expressionist art, then so much the worse for Veblen. Most importantly, it was John Dewey's instrumentalism and not Veblen's instinctually based ethic that was primarily responsible for providing the progressive philosophy and secular humanism that had such an impact on sociologists such as C. H. Cooley and institutional economists such as Wesley Mitchell and Walton Hamilton. Tilman at one point mentions that perhaps he has made Veblen sound more like 'the middle aged John Dewey than he really was' (p. 294). I would go further and argue that in fact it was Dewey, much more than Veblen, who was the person most responsible for the 'progressive enrichment of evolutionary naturalism' that Tilman seeks to trace. The evidence for this is everywhere in the book itself.

There is, however, one other related theme in the present work: that the study of Veblen's evolutionary naturalism has a special contemporary relevance as a 'necessary antidote to the rise in anti-evolutionary thought and supernaturalism that are occurring in American life' (p. 1). With this sentiment I can only heartily agree.

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Andrew B. Trigg, *Marxian Reproduction Schema: Money and Aggregate Demand in a Capitalist Economy.* London: Routledge, 2006. 130 pp. £70.00 (hardback). ISBN 0415-33669-4.

This slender monograph aims to integrate Marxian and Post-Keynesian themes into a unified framework for analysing how finance and aggregate demand affect the operation of a capitalist economy. Although primarily an analytical exercise, the book addresses issues that will be of interest to historians of economics.

Trigg makes a persuasive case for the modern relevance of Marx's theoretical apparatus. The case rests on two significant linkages. The first traces Kalecki's formulation of the principle of effective demand back to the reproduction schema of *Capital*, Volume II; the other concerns the parallels between Marx's monetary analysis and the Post-Keynesian literature on the monetary circuit. The second volume of *Capital* has received considerably less attention than Volumes I and III; Trigg wants to rectify this neglect by showing that Marx's analysis of 'The Process of Circulation of Capital' still has a good deal to teach us about money and economic crisis.

Despite its brevity, the book covers a lot of ground. As is well known, Marx's reproduction schema were inspired by Quesnay's (1764) *Tableau Économique*, which Marx (1893: 364) aptly deemed 'the first systematic conception of capitalist production'. The *Tableau* depicts the economy as a network of interconnected sectors and social classes that collaborate in economic reproduction. Marx's reproduction schema do the same in a more sophisticated way, taking account of the fact that the manufacturing sector is just as capable as agriculture of producing a surplus product. Embedded in Marx's numerical examples of simple and expanded reproduction are a set of sectoral balancing conditions. In order for the economy to reproduce and grow, not only must the level of aggregate demand be sufficient to absorb the monetary value of the economy's output, but the pattern of expenditure needs to jibe with the structural properties of the system.

Trigg neatly lays out the logic of the balancing conditions and then goes on to recast Marx's example of expanded reproduction as an input–output table. When he adds the assumption that commodities exchange at their labour values, Trigg is able to distil the elements of the input–output table into a scalar Keynesian multiplier whose denominator can be interpreted as the share of surplus-value in net income. Although Trigg argues that this multiplier 'captures the inter-departmental structure' of the system, a Leontief-type matrix multiplier would be better suited than a scalar multiplier to the job of showing the interconnectedness of production. Leontief, of course, owed much to Marx's reproduction schema; later work by Richard Goodwin (1949), Miyazawa and Masegi (1963) and Heinz D. Kurz (1985), among others, integrated Keynesian, Kaleckian and Sraffian elements into matrix multipliers that, like Leontief's, show precisely how an autonomous change in some component of final demand ripples through the different sectors of the economy. What do we gain, though, by moving in the opposite direction – by reducing a matrix multiplier to a scalar? For Trigg the advantage of his scalar multiplier is that it 'retains the simplicity of the Keynesian multiplier together with Marx's value categories'. The simplicity argument is valid; but we already have a scalar multiplier. In fact, we have several of them, including a Kaleckian variant that takes account of differences in saving propensities between workers and capitalists, and others that incorporate taxation, international trade and the recursive impact of spending changes on the interest rate. So the real question, which Trigg largely skirts, is why we ought to retain Marx's value categories.

Chapter Three is devoted to 'the Kaleckian principle' – the dictum that capitalists get what they spend, which has roots in Marx's balancing conditions (see Kalecki 1968) and parallels in the widow's cruse parable of Keynes's (1931) *Treatise on Money*. Trigg plays around in interesting ways with the accounting relations that connect the different parts of the economy, to expose the structural similarities of Kalecki's and Marx's conceptions of the economic system. A significant difference emerges in their treatments of investment. For Kalecki, investment includes only expenditures on the output of the capital goods sector; Marx, however, defines investment to include also expenditures on variable capital. The principle that capitalists get what they spend holds in both theoretical systems, but on Marx's definition of investment, capitalist spending includes increases, or more generally changes, in the sum of wages advanced to workers. Adopting this definition, Trigg is able to equate capitalist expenditure with the total amount of surplus-value generated by production, thereby 'establishing a clear role for Marx's theory of surplus value'.

Trigg closes Chapter Three by suggesting that the well-known defects of Marx's value analysis can be resolved if we turn to the literature on 'the value-form'. This interpretive tradition emphasizes the monetary character of capitalist commodity production: since the labour time crystallized in commodities is 'socially validated as value' only through the sale of those commodities for money, any attempt to gauge a commodity's value must reckon with the process of the circulation of money. Hence we cannot determine labour values by solving a system of simultaneous equations *à la* Morishima (1973); for the labour time embodied in a commodity does not become 'value' until the commodity is sold for money. Trigg proposes that his multisectoral formulation of the Kalecki principle accords with the value-form approach by preserving the conception of value as a quantity of embodied labour time and by assigning a meaningful role to money in the analysis of value. He provides no detailed account of how the value-form approach rescues the labour theory of value, however, and the claim is not persuasive. No one denies that a commodity that cannot be sold will fail to realize its value, whether the latter is defined as embodied labour time or as long-period price of production. But this common-sense observation hardly provides the basis for a rehabilitation of Marx's value analysis.

In any case, the problem that really interests Trigg has little to do with the labour theory of value; that is the question of how monetary considerations impinge upon the reproduction and expansion of a capitalist economy. We can trace a more or less clear line of descent from the rudimentary monetary circuit of the *Tableau Économique* through Marx to Rosa Luxemburg and then to the circuitist branch of modern Post-Keynesianism. 'Follow the money' is the guiding maxim of this analytical tradition. How money circulates through the various sectors of the economy is of interest because that process is connected to the emergence of imbalances, specifically financial bottlenecks, that can lead to systemic dysfunctions – crises, unemployment, sluggish growth or unacceptable degrees of income inequality. Using his Marx–Kalecki matrices, Trigg evaluates the various approaches to the question of how much money is needed to sustain any particular production circuit. His analysis supports the circuitist conclusion that capitalism economizes on money balances to a greater degree than Marx supposed. In line with the Kalecki principle, Trigg concludes that the spending circuit can be closed if banks advance to capitalists a sum of money equal to the latter's planned consumption and investment expenditures. He shows also that the circuit generates a Keynes–Kalecki multiplier effect that is of a piece with the velocity of circulation of money.

Trigg's conceptualization of money and the financial system is somewhat underdeveloped, however. He emphasizes that money originates in the extension of credit; but since he does not explicitly incorporate the banking system into his model, there is no discussion of how the money supply is determined. It is one thing to identify the quantity of liquidity needed to enable a circuit of production to unfold smoothly, quite another to show precisely why financial markets often fail to provide it.

Within the Kalecki–Marx tradition, as in post-war mainstream Keynesianism, the fundamental influences on the trajectory of a market economy are the inclination of business firms to undertake investment spending, and the availability of the finance needed to transform planned investment into actual expenditures. Trigg explores these issues in Chapters Five and Six.

Kalecki (1968) noted that post-war theories of economic growth are grounded in the same logic that underpins Marx's expanded reproduction schema. In Chapter Five Trigg looks at this linkage. Unlike Roy Harrod, who incorporated an accelerator mechanism to explain investment, Evsey Domar treated investment as an exogenous variable, an approach that is more closely in tune with Marx's analysis of accumulation. Marx and Domar identified a central condition for continuous balanced growth: since investment spending expands productive capacity, capital accumulation can proceed without a hitch only if demand grows in step with the expansion of capacity. But there is no particular reason to suppose that demand will in fact grow fast enough to ensure the normal utilization of productive capacity. In the end, everything depends upon the vector of final demand – mainly the investment component – which, when multiplied by some variant of the Leontief inverse, ultimately determines the level and composition of production. Kalecki (1967: 453) identified the development of a theory of investment as 'the central problem of the political economy of capitalism'. Trigg sensibly refrains from tackling this ambitious problem, which lies outside the scope of his project, but he does provide a useful overview of how credit and finance bear upon accumulation in the Marxian and Post-Keynesian literature.

Trigg identifies two broad Marxian traditions in early-twentieth-century crisis theory. The first, associated with Tugan-Baranovsky and Rudolf Hilferding, attributes crises to sectoral imbalances – disproportions between the consumption goods and capital goods sectors. The second approach, developed by Rosa Luxemburg (1913), argues that crises result from the difficulties capitalists encounter in their efforts to realize profits from an ever-expanding productive capacity. Although Trigg acknowledges that sectoral imbalances are symptomatic of most crises, he views aggregate demand failures and blocked access to credit as the key causal factors. Accordingly, he favours Luxemburg's approach. Luxemburg argued that the output generated by newly created productive capacity would have to be soaked up by 'external markets', by which she meant non-capitalistic systems or sectors that could absorb surplus output without themselves contributing to the pool of surplus commodities: underdeveloped countries, and, within a capitalist economy, any non-capitalistic zones of activity such as peasant agriculture or the government sector. But since capitalism tends to obliterate the non-capitalist relations it encounters, these external sources of demand will eventually be lost, whereupon the system is bound to collapse as market limits deprive capitalists of the motive to invest. Credit markets, she observed, loosen the financial constraints on accumulation, but exacerbate the boom–bust cycle by enabling capitalists to expand capacity beyond the level that can be supported by aggregate demand.

Chapter Seven is an astute exposition of Henryk Grossmann's effort to expose a connection between Marx's analysis of the tendency of the profit rate to fall and the fragility of the growth process. Where Kalecki treats profits as the by-product of capitalists' spending decisions, Grossmann treats capitalist consumption as a residual determined by how much surplus-value capitalists decide to channel back into capacity expansion. Trigg contrasts Grossmann's prediction of an inevitable breakdown of the system against the results obtained when Kaleckian assumptions are adopted. If capitalist profits are presumed to depend on the expenditures of the capitalist class, with capitalist consumption acting as a driving element, through its role as a component of aggregate demand, Trigg finds that capitalist consumption and the mass of surplus-value both rise over time, though the profit rate, as in Grossmann, falls.

Trigg's Kaleckian focus is reflected in his analysis of Marx's theory of the tendency of the profit rate to fall. The main causal mechanism, according to Trigg, is not the higher rate of increase of the organic composition of capital relative to the increase in the rate of surplus-value, but the difficulty of realizing profits as capital accumulates. Trigg over-reaches a bit when he asserts that 'Marx places realization problems at the centre of his analysis of the falling rate of profit'. The passage from *Capital* that Trigg quotes to support this claim gives a somewhat different impression:

> With the development of the process, which expresses itself in a drop in the rate of profit, the mass of surplus-value thus produced swells to immense dimensions. Now comes the second act of the process. The entire mass of commodities, i.e., the total product, including the portion which replaces the constant and variable capital, and that representing surplus-value, must be sold. If this is not done, or done only in part, or only at prices below the prices of production, the labourer has been indeed exploited, but his exploitation is not realised as such for the capitalist, and this can be bound up with a total or partial failure to realise the surplus-value pressed out of him, indeed even with the partial or total loss of the capital. (Marx 1894: 244)

Realization problems comprise 'the second act of the process'. But the profit rate falls, in Marx's argument, independently of whether the second act is performed. The second act may be extremely likely, but it is not inevitable; and the profit rate, according to Marx, would fall without it. Realization problems exacerbate a crisis that, in this part of Marx's analysis, has been set in motion by an altogether different mechanism. Trigg's idiosyncratic reading of the passage dovetails with his emphasis on finance as a lubricant for investment spending.

In an anticlimactic final chapter, Trigg drops the assumption that money prices are proportional to labour values, and then derives a scalar multiplier that, like the versions presented in earlier chapters, may be interpreted as the reciprocal of the share of surplus-value in aggregate net output. He obtains this result by redefining the value of labour power as the money wage bill multiplied by the ratio of total direct labour time expended in production to the monetary value of aggregate net output – a procedure put forth, under the label of the 'New Interpretation' of the transformation problem, as a way of preserving central elements of the labour theory of value. Whether the difficulties of Marx's value analysis can be rectified by a redefinition of terms is a question that cannot be addressed here. But the attention that Trigg pays to the issue seems misplaced, in as much as one thing his stimulating book makes clear – albeit inadvertently – is that Marx's pioneering and enduring insights on aggregate demand, accumulation and finance need not be grounded in the labour theory of value.

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Karsten von Blumenthal, *Die Steuertheorien der Austrian Economics. Von Menger zu Mises.* Marburg: Metropolis Verlag, 2007. 436 pp. €38.00. ISBN 978-3-89518-618-9.

In Germany's academic environment, the Faculty of Economics of the University of Hamburg has long stood out as one among only few where research into the history of economic thought is still well and alive. This fact has been confirmed by the steady output of doctorate theses in the history of economic thought, some of which have been published by Metropolis Verlag. The latest contribution in this series is the work to be reviewed, by Karsten von Blumenthal on the tax theories of Austrian economics. At first glance, perhaps apart from the early work of Emil Sax, few historians of economic thought might have identified the domain of tax theories as a main focus of the Austrian school of economics. Thus Blumenthal's study addresses a topic hitherto widely neglected by the literature. In fact, this study is not strictly limited to tax theories, but also deals with the evolution of public finance in general. Consequently throughout the book the issues to be surveyed are partitioned into four categories, namely the ideas on the nature of the state, efficiency of taxation, equity of taxation (i.e. principles of just taxation), and, finally, tax incidence.

The structure of the book can be broadly described as starting with propaedeutics and then turning to the different generations of the Austrian school, within which the respective members are examined one by one for their contributions to tax theory. Apart from the introduction, the propaedeutics comprise four chapters, the first and most important one examining whether it is possible to speak of the Austrians as a 'school' in the proper sense of the term (answering it in the affirmative) and which economists are to be counted as its members. Here, the author tends – possibly for the sake of not excluding some economists with important credentials in tax theory – to a rather generous interpretation. For example, not only Schumpeter is associated with the Austrian school but also a still more detached figure like Eugen von Philippovich or mere practitioners with some Austrian origins in their education as economists, like Rudolf Sieghart. The next chapters provide overviews on the state of the art with regard to Austrian tax theory – predictably a rather thin chapter, on the historical background of Austria in its evolution from an empire to a small land-locked country, and finally on the development of tax theory outside of Austrian economics.

Having thus built the foundations, the next two chapters represent the core of the book, summarizing the Austrian contributions to tax theory in chronological order. Of the first two generations the focus is on Carl Menger, Robert Meyer, Sax as the most important, and Friedrich Wieser, followed by lesser-known exponents most of them rightly neglected (Eugen Böhm-Bawerk is only referred to in his capacity of Minister of Finance). Yet although the basic approach of Austrian economics had been shaped by Menger and Wieser, with regard to tax theory Meyer and most notably Sax deserve priority. Both Meyer, in a passage only covering a few pages, and Sax, at monograph length, established what might be judged the only true progressive shift in the Austrian approach towards taxes and public finance, namely the application of the subjective theory of value to the problem of how both to determine the distribution of the tax burden among taxpayers and also the optimum size of the budget by counterbalancing utility gains (from the government's services) and losses (from taxes). This new approach, as correctly pointed out by Blumenthal, only came to fruition fully when developed further by the Swedish economists, Wicksell and Lindahl, yet Sax must be credited at least with having initiated this tradition. However, the shortcoming of Sax's accomplishment in this regard was his awkward distinction between individualism and collectivism as two types of forces inherent in people's motives, with individuals only being able to determine the right size of the budget when acting within (or delegating decision to) a collective body like parliament.

Of the third and fourth generations, only Schumpeter is to be singled out for outstanding contributions. Among these his essay on the crisis of the tax state can be rightly considered a major venture into economic (or financial) sociology and an example of the type of socio-economic analysis he strived for. Furthermore, the integration of the analysis of the tax system into his wider theory of economic development, rendering, for example, a justification for Schumpeter's advocacy of a switch from direct to indirect taxation, or even to a consumption tax, may also be counted among the Austrians' more noteworthy achievements. However, the detailed examination of other contemporary members of the Austrian school, like Ludwig Mises, Hans Mayer, Richard Strigl and Martha Stefanie Braun, leads just to the negative conclusion that with regard to taxes they added little that was novel to the existing body of knowledge.

In his conclusion, Blumenthal gives a somewhat brightly coloured picture of the achievements of the Austrian school. In particular, Sax's application of Austrian subjective value theory to the problem of taxes is raised to the rank of a 'genuinely Austrian' tax theory. Unfortunately, its development was a work of Swedish, not Austrian, economists, and Sax's specific suggestion of distributing the tax burden in such a way that the loss of value is made equivalent for all taxpayers lost its appeal after the Robbinsian revolution that rejected the idea of interpersonal comparisons of utility. Taking into account that – with the exception of Schumpeter – most other works of the Austrian economists on the issue of taxes were either eclectic or a casual response to practical problems, one might after having read this monograph end up with the judgement that to some extent the topic had been neglected by the literature up to now for good reasons.

Thus, the choice of topic of this monograph eventually turned out less rewarding than might have been expected by its author, which to be sure is not the author's fault but his subject's. Quite to the contrary, the work demonstrates the eruditeness of the author, be it with regard to primary or to secondary sources, and indeed it is hardly to be expected that a future investigation into the same topic may turn up evidence not yet treated here. Moreover, apart from an understandable tendency of being a little bit too favourable towards the performance of the Austrian school, Blumenthal's judgement is as a rule even-handed and judicious. Finally, from the artistic point of view, one might wonder whether an approach less centred on persons, and more on subjects, would not have paid off by rendering the book more concise and exciting.

Yet, such minor criticisms notwithstanding, this is a work that in my view will settle its subject, written by an author from whom we are fully entitled to expect yet more fastidious and important explorations into the history of economic thought.

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A response to F. Petri

One must admire Professor Petri's chutzpah. In his 11-page review (Petri 2007) of our (with Christopher Bliss) *Capital Theory* collection, he repeatedly takes us to task because our introduction, which attempts to explain common themes in over 100 years of controversy in capital theory, does not place the long/short-period distinction at the centre. After repeatedly criticizing our work with phrases that begin with something like (p. 604) 'A better grasp of the distinction between the long-period and the very-short-period equilibria would have avoided the authors the mistake of [fill in the blank]', he concludes by suggesting that the reader skip the introduction altogether.

Good academics have passionate beliefs in the correctness of their ideas, and Professor Petri clearly believes that we should have written an introduction that he would have written. Good academics also do not repudiate the primary responsibility of a reviewer – to inform the reader of the arguments of the authors. We wonder whether Professor Petri's combination of suggesting that the reader avoid our work while not even describing our main arguments stems from the arrogance of someone who believes in his monopoly on Truth, or from a desire to hide the arguments from the reader.

For the interested reader, our arguments (Professor Bliss can defend his arguments separately if he so chooses) are outlined in the first few paragraphs (pp. xxvii–viii). Capital theory controversy commonalities originate in economists' conceptions of capital both as a heterogeneous collection of specific capital equipment used in production, and as a homogeneous fund of financial value that flows among alternative uses to establish a uniform rate of return. While economists usually agree that capital had both physical and value conceptions, controversies begin when the dual conceptions are integrated into economic models and one conception is emphasized to the *relative* neglect of the other.

We argue that most capital theory controversies of the past 100 years revolve around two major problems: (1) integrating production into the scarcity theory of value, and (2) integrating capital and time into equilibrium models. Two further commonalities exist in attempts to deal with these problems: (3) the panacea of one-commodity (and putty capital) models in eliminating the tension between the physical and value conceptions of capital, and (4) the role of ideology and vision in fuelling controversy, especially when one-commodity results are not robust.

Anyone who has read our introduction will be able to assess Petri's criticisms of our value/price distinction, which has been championed by Luigi Pasinetti (1974, 1986), and originated with Ronald Meek (1977) (an author Petri regrets we did not include, who extended the impact of Sraffa on interpretations of the labour theory of value). Petri's stress on the neoclassical need for a capital endowment specified as a single quantity of variable form (an insight of Gargnani's (1959) Ph.D. dissertation) is easily subsumed in our argument about the panacea of one-commodity/putty capital models, and illustrated in our documentation of Böhm-Bawerk creation of the putty capital metaphor (he called it 'value jelly') in describing J. B. Clark's 'true capital'. Petri ignores or dismisses our arguments, even though we are both among the small handful of persons who have actually read Garegnani's dissertation and have reasonable familiarity with the Cambridge capital theory controversies (Cohen cedes pride of place there to Harcourt). Moreover, Harcourt published in the *Australian Economic Papers* one of Petri's (1978) first papers, extending the spirit of Garegnani's work in a critique of Walras's and Wicksell's systems. Harcourt (2004) also reviewed in this journal the Petri/Hahn volume of 2003 and applauded Garegnani's ideas *vis-à-vis* Hahn's.

The reader of Petri's review would not have a clue about our discussion of the ways in which so many of the capital theory controversy combatants over the past 100 years (Böhm-Bawerk, Veblen, Knight, Kaldor, Hayek, Hicks and Robinson) have had serious concerns about using differences in equilibrium positions to explain changes over time. Finally, Petri says nothing about our extensive discussion of faith and ideology, which explains many of Petri's disagreements with Professor Bliss's introduction.

The invisibility of these arguments in Petri's review has historical origins in the methodological schism that arose in the late 1970s between Joan Robinson and Sraffians like Garegnani and Petri (another invisible issue in Petri's review). Petri treats most of our arguments – especially those similar to Robinson's – as though they do not exist, in the same way that modern neoclassical authors have treated the Cambridge capital theory controversies as though Sraffa and Robinson did not exist.

We are reminded of Keynes' (1987: 243) question about Hayek's review of *A Treatise on Money*:

> Hayek has not read my book with that measure of 'good will' which an author is entitled to expect of a reader. Until he can do so, he will not see what I mean or know whether I am right. He evidently has a passion which leads him to pick on me, but I am left wondering what that passion is.

What is Petri's passion that leads him to steer readers away from our work? Is it because we exist beyond the pale of the circle that is bounded by those who treat Gargenani's 1990 article on capital as the gospel Truth? Is it because we provide a framework that unifies and explains over a century of capital theory controversy, a framework that is more coherent, compelling and accessible than Petri's long/short-period distinction based on Gargnani's insights?

These are questions not for us, but for you, the reader, to decide. We encourage interested readers to look at Gargenani (1990), and Petri (1978, 2004), but also to look at Bliss et al. (2005) and Cohen and Harcourt (2003a, 2003b), and judge for yourselves.

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Reply to Cohen and Harcourt

The readers of my review have learnt about the contents of professors Cohen and Harcourt's introduction much more than usual, in a review, on the introduction to a volume of readings. Their views that for space reasons I had to leave unmentioned were less central – for the issue that makes the topic so relevant: the consistency of the neoclassical approach – than the analytical deficiencies I criticized. But I am glad to have an opportunity for additional clarifications.

First, it was not Ronald Meek I had in mind when asking for more on Sraffa's impact on the interpretation of the classics; his contributions were admirable for the times, but we are now clearer on the analytical root of the differences between classical and neoclassical approaches (the different explanation of wages, connected with the absence or presence of factor substitution mechanisms allowing the derivation of decreasing demand curves for 'factors', and motivating different choices of data, unknowns and method: *these* are the relevant differences, from which the different theories of value and 'visions' historically resulted). Still, Meek understood and agreed that one of Sraffa's main aims *and results* had been to rehabilitate the classical approach; indeed, he thought that Sraffa had *confirmed* the essence of Marx's approach. Thus, it is certainly not in Meek (nor in Pasinetti) that Cohen and Harcourt can find support for their extravagant view that the basic insights of the classical approach are as in trouble as the neoclassical ones outside the one-commodity model (p. xliv) – a view that renders any preference for either approach unscientific, and thus prepares the ground for the introduction's last part, where they do not question Blaug's determination 'to hang on to a theory [the neoclassical one of course, *F.P.*] despite anomalies if no better rival theory is available' (p. li). Where one wonders why they do not oppose Blaug (on the basis of Joan Robinson and Eatwell's textbook if not of anything else) the existence of a classical-Keynesian approach capable – much more easily that the neoclassical approach – of determining distribution and employment in accordance with empirical evidence, and needing neither a given quantity of value capital nor fairy tales like the auctioneer or complete futures markets – hence clearly a better rival theory.

Second, Cohen and Harcourt object that my stress on the neoclassical need for a conception of capital as a single factor of variable form is 'easily subsumed in our argument about the panacea of one-commodity/putty capital models'. But their identification of the traditional conception of capital (that could adapt its 'form' only gradually, in the long period) with a putty capital that can be 'timelessly remoulded' and is associated with 'instantaneous adjustment' (p. xliii) cannot be accepted; it obscures the realism of earlier neoclassical analyses where no such fairytale notion of capital and no assumption of instantaneous adjustment (similar in its unreality to the auctioneer fiction) were entertained and 'historical, out-of-equilibrium dynamics stories' were part and parcel of normal economics, differently from now. This is just one aspect of a general lack of clarity in their piece on the differences between traditional neoclassical theory and modern very-short-period equilibria. I pointed out that Cohen and Harcourt misrepresent the shift to very-short-period equilibria as one to 'simultaneous equations', thus leaving the nature of these models in the dark (was Wicksell 'simultaneous equations' not too?). I add two more examples: they state (p. *li*) that *all* neoclassical models share 'An inverse, monotonic relation between quantity of capital . . . and rate of interest' (are we then to understand that the Arrow-Debreu model, where no 'quantity of capital' appears, is not neoclassical?); they jump without distinction from Arrow-Debreu prices (based on given endowments of all factors) to the long-period prices of Samuelson's equal-factor-proportions model (that assumes an endogenous determination of the types and quantities of capital goods). Thus the reader cannot but remain utterly confused, and unable to appreciate the present state of neoclassical theory because he is given no clue as to what capital the single factor of variable 'form' made possible to achieve in traditional neoclassical analyses: sufficient persistence of the equilibrium's data, sufficient factor substitutability, no need for complete futures markets or unobservable expectations, stability of the savings-investment market – properties without which the older notion of equilibrium would not have had the explicative and predictive capacity fundamental for the success of the neoclassical approach, and all of them lost with the defensive shift to very-short-period equilibria.

Given the well-known association of one of the authors with the critical side in the Cambridge controversies, the risk was high that readers be misled by this introduction into believing that its confusions, omissions and lack of enthusiasm on the existence of alternatives reflect the present situation of the criticism of neoclassical capital theory. I hope I have contributed to prevent such a misunderstanding.

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